

quarters of an hour. From the data published by Coumbaray we might infer on the hypothesis of circular motion, that the body, whatever its nature, had moved at a distance of about 415,000 miles from the sun's surface, and as we know from the experience afforded by the great comet of 1843, there is nothing improbable in a comet having so passed. Perhaps when the sun's disc is more systematically and widely watched, a comet may be caught in transit and properly observed. The case of the comet of 1819 is not a satisfactory one, Pastoroff's observation at least attributing to it a position upon the sun's disc which it could not have occupied at the time he assigns to his observation.

COMET 1874 (II).—The comet detected by M. Coggia at Marseilles on April 17, 1874, which presented so fine an appearance in our northern heavens in July, was observed at Melbourne, and by Mr. Tebbutt, near Sydney, until the end of the first week in October. Comparing the Melbourne observation on the 6th of this month with the place given by the elliptic elements of Prof. Tietjen, the difference is found to be less than a minute of arc, and the European observations to the middle of July are very accurately represented by these elements. Between April 17th and October 6th the comet traversed an arc of 205° of true anomaly, and the near agreement of Prof. Tietjen's orbit throughout, shows that the comet when it attracted so much attention was really moving in an ellipse of very long period, though no doubt this element may be considerably varied without largely increasing the differences between calculation and observation. The period of revolution in Tietjen's ellipse is nearly 9,000 years. When a similar complete investigation has been made for this comet to that so skilfully performed by Dr. von Asten in the case of Donati's great comet of 1858, some kind of limits may be assigned to the time of revolution, but in all probability it must extend to some thousands of years. We remark that the Melbourne observations of Coggia's comet were made with a telescope of only $4\frac{1}{2}$ inches aperture; no doubt the comet might have been followed some time longer with larger instruments, but it is possible that the Melbourne reflector may have been under preparation for the transit of Venus, and not conveniently available for cometary observations.

PROF. LOOMIS ON THE U.S. WEATHER MAPS *

THIS paper is in continuation of a similar paper published in July last year, in which the American Weather Maps for 1872-73 were discussed. The results then arrived at are compared with the observations of 1874, and the whole is thereafter combined into a three years' average.

The principal conclusions from the three years' observations are these:—

The mean direction of the onward course of storms is $N. 81^{\circ} E.$, or a little to the north of east, being most southerly in July ($E. 7^{\circ} S.$), and most northerly in April and October ($N. 72^{\circ} E.$ and $N. 74^{\circ} E.$). The mean velocity is 26 miles per hour—the maximum monthly velocity, 32 miles, being in February, and the minimum 18.4 miles in August. As regards particular storms, wide deviations from these figures take place, it being found that the actual motion of the storm's centre may have a path in any direction whatever, and the velocity of progress may vary from 15 miles per hour towards the west, to 60 miles per hour towards the east. From the tri-daily observations it is found that the average velocity of storms from 4.35 P.M. to 11 P.M. is about 25 per cent. greater than for the rest of the day, and that while this

* Results derived from an examination of the United States Weather Maps for 1872-73-74. By Prof. Elias Loomis, Yale College. From the *American Journal of Science and Arts*, vol. x., July 1875.

varies in different months from 14 to 32 per cent., the most rapid progress occurs in every month during this portion of the day. Prof. Loomis suggests that as this is the time of the day when the temperature is falling most rapidly, the fall of rain may be thereby accelerated, and the velocity of the storms' progress be increased by the more rapid extension of the rain-area which would follow. The meteorological system of the States fortunately furnishes the required data for the examination of this important point, and we shall look forward with great interest to discussions of the daily rainfall of the States in this connection.

It would appear that an unusual extension of the rain-area of a storm is generally accompanied by a velocity of progress greater than the mean. The average extent of the rain-area eastward from the centre of the storm is 542 miles; but when the eastern extent of this area is 100 miles greater than the mean, the hourly velocity of the storm's progress is increased $13\frac{2}{3}$ miles; and when on the other hand, the eastern extent of the rain-area is 100 miles less than the mean, the hourly velocity of progress is diminished $9\frac{1}{2}$ miles. Whilst the extent of the rain-area exercises an important influence on the storm's progress, the inclination of its axis would also appear to influence to some extent the course of the storm. Professor Loomis is of opinion that the direction and velocity of the storm's progress may be predicted with some confidence, in cases when the precise limits of the rain-area are known. It is thus most desirable that rain observations form an integral part of all weather telegrams.

The influence of areas of high barometer on the velocity and direction of a storm's course is important in connection with the prediction and theory of storms, but further observations are required for its elucidation, among the more important of which are the movements of the upper currents of the atmosphere as disclosed by observations of the cirrus cloud.

The reports of General Myer, Chief Signal Officer, for 1872-73-74 show by the barometric results for Denver and the other elevated stations on the spurs of the Rocky Mountains, that the relative distribution of atmospheric pressure at these great heights is just the reverse in summer and winter of what obtains at lower levels to eastward in these respective seasons. The point is a vitally important one in its bearings on the weather and meteorology of the States. In connection with it, we have examined with much interest the tables at pp. 10 and 11 which give the number of times during 1873 and 1874 on which the daily change of temperature amounted at the different stations to 40° and upwards. This large temperature fluctuation occurs most frequently at Colorado Springs, Denver, and the other high stations in the west. The most remarkable of these changes occurred at Denver on the 14th of January 1875, at which place the temperature was below zero all day, and the wind N.E. At 9 P.M., the temperature was $1^{\circ}0$ and the wind suddenly shifted to S.W.; at 9.15 P.M., the temperature had risen to $20^{\circ}0$ at 9.20 P.M. to $27^{\circ}0$; at 9.30 P.M., to $36^{\circ}0$; and at 9.35 P.M., to $40^{\circ}0$, after which there was little change till the following morning. At 11.30 A.M. of the 15th, the temperature was $52^{\circ}0$ and at this time the wind suddenly backed to N.E.; at 12.30 P.M., the temperature had fallen to $4^{\circ}0$. Thus in the evening of the 14th, the temperature rose $39^{\circ}0$ at Denver in the short space of 35 minutes, and about noon of the following day fell $48^{\circ}0$ in one hour.

ON THE HORIZONTAL PHOTOGRAPHIC TELESCOPE OF LONG FOCUS *

IN what I have now to say in regard to the methods of Photography employed in observing the recent Transit of Venus, I shall confine myself to the subject of

* This paper was read by the late Prof. Wenlock to a private scientific Club in Cambridge, U.S., shortly before his death; it has been forwarded to us for publication, at the request of the Club, by Prof. Asa Gray.

the instruments used, having especially in view the explanation of the advantages of the horizontal telescope and its origin.

I should not have thought it worth while to make any communication on this subject, but since it has been a matter of discussion in the French Academy, and several pamphlets have been written on the subject, it may not be uninteresting to explain here the connection of the Observatory of Harvard College with it.

In the spring of 1869, when it became necessary to begin preparations for observations of the Solar Eclipse of August 7th of that year, my attention was called especially to the subject of Solar Photography for the first time. Mr. Warren De La Rue in England, and Mr. Rutherford in America, had devoted themselves almost exclusively to astronomical photography for many years, and they were the authorities on this subject. The methods employed by them were substantially the same ; each used an equatorial telescope with clock movement, and enlarged the image formed by the object-glass by means of another system of lenses, and photographed this magnified image. Mr. De La Rue corrected his object-glass to secure as nearly as possible the foci of chemical and visual rays, and Mr. Rutherford corrected his so as to obtain the best echromatic combination of the chemical rays without regard to the visual focus.

Mr. De La Rue first undertook a series of daily photographs of the sun at Kew some time previous to 1860. In making my own preparations for the Solar Eclipse of 1869, I collected what information I could about the extent and brightness of the corona, and the nature of Mr. De La Rue's photoheliograph which he employed in observing the total eclipse of 1860 in Spain. In the first part of the volume of the "Philosophical Transactions" for 1869, I found a paper which was read May 31, 1868, which contains the results of the first attempt to measure the heliographical positions and areas of sun-spots observed with the Kew photoheliograph. From the examinations of the measurements in this paper I became convinced that no trustworthy measures of photographs taken in this way could be made. The magnified image is so much distorted by the eyepiece, or the equivalent system of lenses used to form the image in the camera, that no satisfactory scale could be obtained ; in fact the scale was found to vary irregularly from the centre to the circumference of the image ; and even if this irregular scale could be investigated, a slight displacement of the centre of the picture from the axis of the telescope would introduce confusion. Mr. De La Rue's method of investigation consisted in photographing the pinnacle of a pagoda which was composed of rings and chains of known dimensions, and then attempting to find the scales of the different parts of the pictures from the images of this pinnacle.

The result, as I have said, satisfied me that this was a method to be avoided. The difficulty arising from the distortion of the image, and the apprehension that the light of the corona might be so enfeebled by enlargement that it would not make an impression on the plate, determined me to photograph the image in the principal focus of the object-glass.

All of the many other parties fitted out for photographing the total phase of this eclipse followed the method of De la Rue and Rutherford ; the expedition from the Observatory of Harvard College was the only one that succeeded in getting a picture of the corona. The method of De la Rue was employed in the preceding eclipse of 1868, and no photograph of the corona was secured. I mention these facts simply to show how little the disadvantages of enlarging the image by an eye-piece were appreciated. In the next eclipse no party went into the field with De la Rue's plan ; every one of them photographed in the principal focus, but this time, on account

of the weather, the American party in Spain alone succeeded in getting the corona.

In 1871, in India, this method was again followed by all the parties, and was successful. My preparatory experiments in 1869 were made with an equatorial of 7 feet focal length, which gave an image of about three-fourths of an inch, and with the great equatorial of 24 feet, which gave an image of $2\frac{1}{2}$ inches diameter.

Measurements of photographs of the smaller image seemed to indicate that under a microscope an accuracy comparable with that of the best meridian circles was attainable ; but believing that a larger image would be better, I thought that four inches would be a convenient size. In order to get such an image free from the distortion of the eye-piece, I must have a telescope 40 feet in length. Immediately on my return from the eclipse of 1869, I ordered a lens of Messrs. Clark and Sons, of 40 feet focus, and a micrometer capable of measuring conveniently an image of four inches diameter. Thus the long telescope was adopted to escape the distortion.

Then of course the difficulty of mounting and handling a telescope of this length, especially when extreme precision in measuring was the main object, naturally presented itself. To obviate this I resorted to the very simple expedient of placing the telescope horizontally, so that it need not be moved at all, and reflecting the light of the sun through it by means of a plane mirror. This seemed likely to meet all of the difficulties of the case ; the well-known methods of mounting and directing collimators rendered the utmost degree of accuracy attainable in directing such a telescope, and by putting the object-glass on one pier and the camera on another, using a tube which should touch neither, only for excluding the light, all disturbance of the focus by the expansion of the long tube was avoided.

Other information obtained by my preparatory experiments had an important bearing upon my plan at this time. I had found how difficult it was to get an exposure of the plate short enough. It became necessary to reduce the apertures of the equatorials to one or two inches, and then throw the slide across with a strong spring. From this I derived two important suggestions : one, that a heliostat driven by clockwork was not indispensable, as the picture would be instantaneous, so that the motion of the sun during the exposure would be of no consequence. The other was that I might reduce the light by using a transparent glass reflector, and not be compelled to reduce the aperture of the telescope so much. By these means the cost of the experiment was greatly reduced, saving the expense both of a heliostat and of a silvered mirror. Messrs. Clark and Sons did not get the apparatus ready for use at the Observatory until July 1870, although it was tried at their shop previously. A series of daily photographs was begun with it, July 4, 1870, and has been kept up with little interruption to the present time.

At this time and for a year or two after, I had not heard of this method being thought of by any one. No one of my acquaintances seemed to have any knowledge of any other claimant of the method. Mr. Rutherford, with whom I had frequent communication, and who had been occupied with the subject for twenty years, regarded it as new and original. It was described in Mr. Lockyer's paper in 1870, and attributed to me ; Mr. Newcomb, in the latter part of 1872, speaks of it as a method devised by me, and in successful operation for several years, and also independently proposed by Faye. Lord Lindsay adopted it for his expedition to the Mauritius. Mr. De La Rue in several communications down to 1873 spoke of it as the method of the American Astronomers. It was afterwards, about this time, called the method invented by Foucault and Prof. Winlock independently. Then, in 1873, I received a book by M. Edmond Dubois, claiming it as a French

invention, and giving the whole credit to Capt. Laussedat, closing with the remark that if it should be successful the glory would belong to France. Afterwards I received a pamphlet of twenty-six pages, by Capt. Laussedat, in which, ignoring me entirely, he tried to sustain his claims against those of Faye, Foucault, and Fizeau.

In 1873, after the horizontal telescope had been in successful operation for three years, after specimens, both negatives and lithographic copies had been distributed in Europe, the French Commission, which had up to this time been making their preparations to use the method of de La Rue, adopted the horizontal telescope. It would appear from this, that whatever might have been done or said on this subject by the Frenchmen named above, it had not contributed much to a clear appreciation of the advantages of the method until after they had been demonstrated here.

Without caring anything about credit for priority of suggestion in such matters, being satisfied that no similar instrument was in use or had been used before the one at Harvard College, I was yet interested enough in the matter to look up the claims put forth by these gentlemen, and to see why they happened to be overlooked for so long a time, even by their own countrymen. I find that credit is accorded to Foucault, mainly for his perfection of the heliostat, both for the plane mirror and for the uniform motion. He published nothing in regard to its application to photography. After his death his friend, St. Claire Deville, spoke of it as one of the things that Foucault intended to do. He at the same time contemplated the use of the siderostat in all kinds of astronomical observations. M. Laussedat is unwilling to give him any share of credit for the horizontal telescope. M. Faye gives him credit only for the heliostat.

M. Faye himself took some photographs of the sun with a very long telescope of one M. Porro, of 15 meters focal length. The telescope was pointed directly at the sun. M. Faye's remarks on them before the Academy related only to the advantage of their size and their distinctness. He had nothing to say about the peculiar advantages of the long telescope, but he anticipated all succeeding inventions in the application of Photography to astronomy by predicting its early use in meridian and every other class of observations.

His next communication on this subject was on March 14, 1870, on the occasion of presenting a letter from M. Laussedat on the subject of a horizontal telescope. This was six months after my apparatus was ordered, and after some experiments had been made with it. In this communication he appears at first glance to have suggested the whole arrangement now adopted; but on closer examination he does not seem to have had any clear ideas about it. He recommends the use of a long telescope because he had seen good pictures with a long telescope; he nowhere speaks of his reasons for dispensing with the eyepiece, and in fact it does not clearly appear that he did dispense with it. In September, 1872, after it had been in use for two years and several accounts of it had been published, in his comments before the Academy on a paper of Warren De la Rue's, he seems to have understood for the first time the true theory of the long horizontal telescope.

Capt. Laussedat appears to have the most substantial claim of any that have been mentioned thus far. He used a horizontal telescope in Algeria in 1860, in observing the total eclipse of that year; but he used a very short telescope and had an eyepiece to enlarge and distort the image. His own account of what led him to this method was that he had no equatorial mounting for his little telescope and that no means were furnished him to buy one, but he had a good heliostat, and he resorted to the method as a makeshift. He fully appreciated, however, the advantages over the other method in the accuracy of

orientation and in the certainty with which fixed lines of reference could be had on the plates.

M. Faye, in his communication of Sept. 1872, seriously claims that his use of the long telescope pointed to the sun in 1858—because M. Porro happened to have one, and Capt. Laussedat's use of a short one, placed horizontal, because he had no equatorial stand and clock movement—together make up the invention of the telescope as it is now used.

But, after all that has been said about the priority of suggestion, that question is settled finally by some one* in England finding that the whole arrangement was suggested by Hooke in 1676. A late communication on the subject in the *New York Times* calls it a method suggested by Hooke and perfected by Foucault.

In Hooke's day they had none but very long telescopes, but they had no heliostats. No practical application of his suggestion, however, seems to have been made.

ON THE CARDIOGRAPH TRACE

BY placing the sphygmograph, as constructed by M. Marey, over that portion of the chest where the heart can be best felt beating, instead of on the wrist-pulse for which the instrument is constructed, tracings called cardiograms can be obtained which bring to light physiological facts not otherwise ascertainable. In the last-published volume of the Guy's Hospital Reports there is a paper by Dr. Galabin, on the interpretation of these tracings, which will be read with interest by physiologists on account of the considerable difficulty there is connected with all attempts to explain the numerous ups and downs which they present between any two pulsations of the heart, and also because of the comparatively slight attention which they have had paid to them.

Dr. Galabin, in the paper under consideration, limits his observations almost entirely to the vertical variations in the curves under consideration, paying but little attention to the differences in the relative lengths of systole and diastole which they so clearly indicate, and which cannot be recognised with any degree of accuracy by any other means at our disposal. From a study of the cardiograph trace, he is led to the conclusion that the two most important elevations in the systolic portion of each curve are produced by the muscular movements in the heart itself, because "the more the heart is hypertrophied (by disease) the more prominent in comparison do these two become," and under these circumstances, "the effect of any oscillations, either of the blood or of any solid structures, would become less noticeable in proportion." It is remarked that "Marey's figures (of tracings indicating intracardial pressures) prove that the first, at any rate, of the cardiac impulse is not due to any stroke against the ribs caused by locomotion of the heart as a whole, which could only commence after the opening of the semilunar valves," because "the aortic valves do not open until the ventricular pressure has nearly reached its first maximum." It must, however, be noted that other tracings, obtained by the same illustrious physiologist, demonstrate equally clearly that the maximum of intracardial pressure is reached some appreciable time before the first major systolic cardiograph rise in the trace from the chest-wall, so that it may still be reasonably argued that the rise referred to depends upon the locomotion of the heart *en masse*.

To explain the second main systolic rise, Dr. Galabin makes a statement which needs considerably more demonstration before it can be considered to be proved. He refers to "inverted tracings," by which are understood curves in which all the rises in an ordinary trace are represented by depressions, in such a way that "to see more clearly their correspondence with positive tracings

* A correspondent in NATURE.—ED.